Nuclear Science for the Manhattan Project

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Abstract. This is a transcript of my talk for Nuclear Data for Science and Technology conference, ND2022. The talk is available on YouTube: www.youtube.com/watch?v=a_WYcTHco00. This work is derived from a 2021 paper I wrote in the American Nuclear Society’s journal Nuclear Technology 207, S24 (2021). It was published in a special issue together with twenty-three other papers, for the 75th Anniversary of the culmination of the Manhattan Project. It would be better to watch the video than read this transcript!

Thank you for joining me today for this talk on the history of nuclear fission from 1939 to 1945. I am going to be talking about the remarkable story of the scientists, some of the best in the world together with their graduate students, who came to Los Alamos to support the war effort and develop an atomic bomb. This work was put together in recent years on the 75th anniversary of the culmination of that project in 1945 [1]. We've published many papers in proceedings with the American Nuclear Society and that whole proceeding is available to you as an open access journal [2].

I'll begin by talking about the first conference that was held and chaired by Oppenheimer at the beginning of the Manhattan Project in April 1943. Here I am showing an agenda from that conference. Frankly, it must have been one of the most exciting conferences ever in terms of the technical topics being addressed, the breakthrough physics that needed to be solved and the quality and fame of the scientists. You can see many names who are well known to you including Oppenheimer, Bethe, Konopinski, Christy, Teller, etc. One of them I'll emphasize is the second one, Manley. He is not as famous as some of the others but he's famous in Los Alamos. He was one of their organizational leaders and a very good nuclear physicist who led this effort.

My focus will be on the first three topics here. What was known at the time in terms of cross-sections and criticality, what was needed in terms of experimental capabilities and then some of the criticality calculations that people like Bethe were also working. This was a time when a capability was rapidly put in place and involved carting four accelerators up the hill to Los Alamos in 1943. Robert Wilson was one of the early leaders of this experimental effort and there's a quote by him that I think is quite insightful. “We were constantly plagued by worry about some unpredicted or overlooked mechanism of nuclear physics which might render our program unsound... this tendency prompted us to do many experiments that were otherwise not particularly justifiable; these turned out frequently to be very profitable investments.” The lesson for us then, and the lesson for us today, is difficult problems require many ways to try to solve them; they require different type of experimental approaches with different systematic errors.

They were very insightful in how they pursued these different types of approaches. I list here the different equipment that was brought up to Los Alamos: the two electrostatic Van de Graaff accelerators from Wisconsin, with the Li(p,n) source reaction that had been developed that turned out to be very important for making neutrons in the fast MeV range that's especially important for this application. But we also had other accelerators brought from Illinois, from Harvard and then I mention also other neutron sources. There was a water boiler reactor that created thermal neutrons, and this was the world's third nuclear reactor after Chicago and Oak Ridge, but it was also the world's first solution reactor and first reactor that used enriched uranium. We also had radioactive sample sources that created neutrons through reactions such as beryllium neutron production from photonuclear static sources that were commonly used at the time.

Let me just take you back to some of the very early history. This is from Serber's book called The Primer, which is a very nice book on the history of this physics and it summarizes what they knew in April of 1943. In the picture on the right, you see Serber on the left with other famous people such as Metropolis and Carson Mark, and on the left is a plot of their understanding of the fission cross-sections at the beginning of the project back in 1943, and this isn't far from the truth. 49 at the top is their code word for plutonium; 25 is uranium-235 and you can see that they thought the fission cross-section for plutonium was about three barns. We’ll later learn that in fact that's was too optimistic an assessment, and likewise the uranium cross-section was too optimistic here. From these cross-sections they

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would estimate critical masses and these notes here are taken from the summary of that meeting. They were Oppenheimer's notes on the assessment of the critical mass and in the top on the left is a bare critical mass formula that they used, an analytic formula that turned out to be rather accurate as we'll see. This formula has its origins going back to Peierls and maybe even earlier from work in Birmingham that was published in the Cambridge Philosophical Society Proceedings in 1939.

It was used by the British scientists in their Maud committee, which was the committee that oversaw the British early development of an atomic bomb and then it was adapted in America. The exact history of this formula I won't go into in this talk, but one of the interesting things about this formula is that you can put in typical cross-sections and they're shown in the middle of this slide of estimates. These are one group estimates, just typical cross-sections at relevant energies which might be about one and a half MeV and what we'll see is if you put those numbers into that formula you get answers like 60 kilograms and that's not far from the true value near 47 kilograms that we have today from our best estimates from our transport code MCNP and our fundamental data like ENDF, and integral experiments that we've done since that time to calibrate these kinds of data - notably the Godiva experiment which was done after the war. Lower, I also showed formula they had for tamped critical masses which were typically about half of the bare critical mass and of course the fact that tamped critical masses are smaller was attractive because you needed less nuclear material. If we see how they compare against today, our best estimate for a very sophisticated calculation for an idealized spherical pure 235 uranium system is 46.4 kilograms. If we do a simple thing with the formula that I just showed you, and take the one group cross sections from ENDF near 1.5 MeV, you can see that that formula does very well. It gives 55 kilograms - that's only 20 percent too high, but when you think about it typically these formulas actually calculate the critical radius and then take the cube of that for the critical mass, and so the critical radius is only six percent high. So, really remarkably accurate, and that was back 1943.

By 1944 some of the characters you see here are from a picture from Los Alamos from that era including von Neumann on the left and Feynman in the middle and Ulam on the right. They were all improving the neutronics treatments to include multi-group inelastic scattering, polynomial angular treatments, etc. The story of how the critical mass estimates changed during the project is really a fun story. It involves changes in pessimism and optimism by the scientists as they learned more, as they got better, and as they got worse. For example, this graph here shows the changes in estimates for an idealized plutonium system at critical in the alpha phase, which means the high-density phase, and if you look on the right hand side you'll see our best estimates today near 10 kilograms for that critical mass. When they started off, they were pretty close: you can see the first estimates were just a few kilograms above 10. But then new data came in and in fact one of the first measurements that came in involved a measurement on uranium by Fermi from Chicago and the uranium nubar value was adopted from that experiment for plutonium which isn't a terrible approximation. Fermi was a genius and did everything almost perfectly, but this is a case where he didn't get the right answer. The nubar multiplicity that he got led to a critical mass increase that was very depressing for everybody. So, by the summer of 1943 they were computing that much larger amounts of plutonium that were needed, which of course was a major problem for them in terms of plutonium production requirements from reactors. But then the red arrow, with time, shows that it comes down to a lower result, and people like Segre, shown on the left here, played an important role in determining accurate data. Eventually critical mass calculations transitioned to estimates that are pretty close to what we have today.

On the right-hand side, I'm showing Robert Bacher. He was the first division leader of the Physics Division and he would report progress every couple of weeks to the governing body at Los Alamos, chaired by Oppenheimer, and the notes show times when he had to talk about how cross-sections have changed and unfortunately the critical mass estimates were increasing, and that led to real pessimism.

Right at the beginning John Manley who I previously mentioned, laid out an experimental research program that was needed. I won't go through this in great detail I'll let you read it as I speak, but what he recognized was that what we knew at the beginning of the project wasn't anywhere near accurate enough, and we needed to have systematic experimental approaches, and many different experimental methods, to determine fission cross-sections, fission spectra, multiplicities (that is, nubar) as well as transport cross-sections, and he was the driving force behind the experimental programs that were put in place at Los Alamos. He later became an associate lab director at Los Alamos.

Now we're going to show a picture of the uranium-235 fission cross-section, and this graph was actually made a couple of years after the war in 1947 when Robert Wilson, on the left-hand side, had become the division leader for the Research Division (that was previously called the Physics Division) and at that point the data here - all the data shown in black - were much better understood, and you can see that this fission cross section has the properties that is fairly flat at an MeV or two. That was actually fairly fortuitous because many of their neutron sources were not monoenergetic. They were broad neutron sources, but they were still able to get a reasonable value and as you go down in energy further left on this graph, the cross-section increases as we well know because of the increased absorption at low energies. Now, the coloured points on here represent the early measurements that they had at the beginning of the project, and I've emphasized a few different ones. One that you can't even see on the graph but it's in the top written as Frisch and Peierls, was in 1940 from their guess - they had a 10 barn bad guess that was way too high. There's an interesting story about that guess and
why they got it so wrong, but it led to them estimating a very low critical mass of about 0.6 kilograms which in some sense was fortuitously low because it was amount that they thought was plausible to create with separation processes. If they'd got it wrong in the other direction one wonders whether the project would have preceded. Then we can see some of the early UK measurements in red that were getting better with time. Some of them fairly close to the true answer, and we see measurements from Berkeley from Segre and from the Carnegie Institution in Washington DC by Heidenberg, which were a little bit low here, and the data from Wisconsin from the Van de Graaff which sort of straddled the true answer. So that was sort of where we started.

Now I'll move on to where we ended up. This is a graph of the time dependence of the measurements of this cross-section. So now I'm just picking out one incident energy for neutrons which is about 1.5 MeV here, and I'm looking at how estimates of the 235U fission cross-section changed over time, and what you can see is that early on the data typically were too high and that's probably because there were under-appreciated scattering problems that led to backgrounds at lower neutron energy causing more fission than should have been happening, and an overestimate of the cross section. You can see that the British measurements started to improve and get lower values and then on the right-hand side of the graph are all the measurements from Los Alamos that were made during the war, and by the end of the war in 1945, you can see that the best values there were really within five percent-ish of the true answer. Right on the very end I show the current ENDF evaluation and prior to that I show one just one experiment which was an accurate measurement done by Allan Carson from NIST.

The pictures here on the left-hand side show Peierls and Frisch, who played such an important early role in this work. Frisch and Peierls of course were also scientists at Los Alamos during the war, and on the right-hand side I show a picture of Theodore Hall. He was one of the very young experimentalists who came to Los Alamos. I think he might have been not much more than 18 years old and he played an important part in doing these experiments, co-authoring some of the papers that show the most accurate measurements of cross-sections during the Manhattan Project. So that was quite an accomplishment for a young scientist. Unfortunately, he was also a spy and was giving this information to Russia: that was learned after the war. Of course, many people know Klaus Fuchs as the more famous atomic spy during this era, but Theodore Hall was also a spy but also a remarkable nuclear scientist in the same kind of way that Fuchs was a remarkable scientist as well.

So, there we saw the fission cross-section decreasing with time, which is sort of bad news in terms of what the implications are for critical mass. Fortunately, the story for nubar was the opposite. The early estimates were too low, and as they obtained better and better measurements, they approached a higher value which really helped in terms of the critical mass measurements. I'd already mentioned the kind of the wild goose chase in the middle of this where Fermi got a lower value that turns out to be incorrect and was later improved by more accurate measurements.

So now we'll move on to the fission spectrum. Again, the picture here is from Serber's Primer and it shows what they thought the answer was at the beginning of the project. They thought correctly that there was a spectrum that peaked about one MeV and had a mean average energy of about 2 MeV. Now here they're not being very precise on what this spectrum represents. Was it thermal, was it fast, was it uranium, was it plutonium? At their point of research, they thought that the differences in spectra based on incident neutron energy and based on uranium versus plutonium targets wouldn't be very big. They were correct. So, they were able to not worry about that too much and here we'll find that there's a fascinating story about how this estimate has changed with time. Very early on Bethe also developed some insightful thinking in how the fission spectrum works, how the evaporated neutrons get boosted into the lab frame by the moving fission fragments, and this paragraph, which I won't read verbatim, but I'll let you read, describes his understanding. He was trying to explain why the right energy for the average energy, for 235-uranium, might be around 1.7 MeV. Now he was doing that because there'd been a recent measurement by Bloch from Stanford that got 1.7 MeV for that energy.

The picture on the bottom right is from a paper from Britain by Norman Feather, written very early on, where Feather came up with a theory of how the fission spectrum and kinematical boosting process works, and it's a beautiful paper. You can see that they were thinking all the right kind of thoughts about how the fission spectra should be understood. By the end of the project, they'd accrued quite a lot of different data sets, so the only data set that's not during the Manhattan Project is the red data and this is from the recent Chi-nu experiment at Los Alamos, at LANSCE, that's been done by Los Alamos and Livermore and recently published. We think that's quite an accurate measurement. The French CEA lab collaborators have also recently measured it and got very similar results, so there's confidence gained by the agreement between those two experiments, and the other data you see are from different attempted measurements. Some of them before Los Alamos started - the early work came from experiments at places like Rice and Minnesota and Stanford. By the end of the project, Staub and Nicodemus' data were rather accurate, and so by the end of the project they had a much better understanding. I'm plotting the time dependence in the measurements from the beginning to today.

In 1943, Serber thought the answer for the average energy was about 2 MeV, and you can see in the middle of this graph he was basing that on some of the earlier estimates that are shown to the left: one was this measurement from Stanford by Bloch giving 1.7 MeV
average energy; another was a very clever integral experiment by Christy and Manley done at Chicago, where they got something like 2.2 MeV and they knew that that number would be an overestimate. They stated that there were some other measurements which were less accurate, but they were the first ever, by people collaborating with Chadwick at Liverpool and collaborators at Rice, and they tended to get much larger average energies because of some systematic errors.

Their answer has changed from 2.00 MeV in 1943 to ... 2.00 MeV today which comes out of the most recent IAEA assessment for the thermal uranium fission spectrum. So, it's rather remarkable that we've ended where we started.

Now, we'll talk about the transport cross-section. Transport involves the sum of nonelastic and elastic scattering processes. In that early work from the Maude committee from Britain, where Chadwick is shown on the left, a very nice estimate is given for what we call the transport cross-section, or what they call the collision cross-section. For uranium, they'd estimated a number between 3.5 and 5 barns. We know from our very best understanding today that the answer is 4.85 barns, and so that's really quite a remarkable agreement.

Here's a graph of the transport cross-section, showing on the right-hand side where we've ended where we began. On the left hand side we have a number near five barns for the transport cross-section, from the early 1940s. Heinz Barschall, who did the early work, is shown here in this picture. He spent some years at Livermore, and this picture was taken at Livermore, and I had the pleasure of working with him early in my career when we were both interested in neutron cross sections for medical applications.

I'm coming to the summary here and showing how the evolving understanding of fission data and transport data led to improved cross-sections. By the end of the project many of these cross-sections for fission were determined (in the fast region) to better than five to ten percent. That's really a remarkable accomplishment in just a couple of years of extraordinary intense effort. The big error bar on the uranium critical mass, shown in the beginning in 1943, gives my attempt at putting an error bar on Oppenheimer's first estimate that we showed at the beginning of the talk of 60 kilograms, see Fig. 1. The critical mass error bars are massive because when I look at the uncertainties and how far wrong they were (initially) on the underlying cross-sections, you can get that large range of estimates of the critical mass. But as the cross-sections were improved, they obtained uncertainty values much smaller, and I'm showing one by March 1945 where you see the upper error bar about 80 kilograms - so it's come down quite a lot. I'm showing our best estimates today, that come from our best nuclear data, with MCNP transport, but we also do calibrate to the integral Godiva experiment that came after the war, around 1952. In fact, it's worth noting that during the war itself we did do criticality experiments as described by Jesson Hutchinson in his nice paper in the ANS proceedings. But we didn't have enough material to actually perform bare critical mass experiments, so they were all tamped critical mass experiments. The progress, both the integral experiments and simulation-based cross-section calculations, was remarkable.

Fig. 1. The calculated \(^{235}\text{U}\) bare critical mass with uncertainties - based on differential cross section data - at the beginning of Project Y, 60 (+210, -33) kg and at the end of Project Y, 66 +/- 14 kg as well as from the first integral experiment measurements at Los Alamos in 1945 as described by Hutchinson et al., 50.8 +/- 2.5 kg (solid circle point). The horizontal line represents our best understanding today from MCNP6 simulations using ENDF/B-VIII.0, 46.4 +/- 1.7 kg, which were in practice calibrated to the Godiva bare HEU critical mass experiment in the 1950s.

Thank you for your attention during this talk. I'll end by simply putting up some of the key sources of information that have helped me. Maybe I'll just emphasize one of them, which is the third on the list. We are lucky to have an archive at the Los Alamos that holds many historical documents, our National Security Research Center (NSRC). That allowed me to dig up many of the original sources related to this work.

As a final teaser I'll say that the next project we're working is the technical history of fusion. Within a few years I hope to be able to talk to you about nuclear fusion cross-section breakthroughs, both during the Manhattan Project at Los Alamos and then in the late 1940s and early 1950s, on fusion DD and DT breakthroughs, so please stay tuned. Thank you.

References
